disjoined. Thus it is that, even during the history of an individual intelligence, "practice makes perfect," by frequently repeating the needful stimulations along the same lines of cerebral discharge—so rendering the latter even more and more permeable by use. Thus it is that a child learns its lessons by frequently repeating them; and thus it is that all our knowledge is accumulated. In a word, if, as has been truly said, "man is a bundle of habits," we have in Mr. Spencer's theory of nervo-genesis a physical explanation of the fact. And forasmuch as it is upon this theory that Mr. Spencer may be said to found that great monument of modern thought—his "Principles of Psychology," I cannot but feel that one of the most important bearings which my work on the Medusæ has had, is that of supplying facts which tend to substantiate this theory—and this at a time when it seemed as though the theory could never have other than *à priori* considerations for its support. But if my interpretation of these facts is correct, this important theory is now receiving inductive verification from a most unexpected source. At first sight no two organic structures could well seem to have less in common than the swimming-bell of a Medusa and the brain of a Man; nor could anything seem more unlikely than that a great psychological theory should derive support from the study of polypes, where the very existence of a nervous system has only just been discovered. But here again, I believe, we may discern the uniformity of Nature; and while watching the passage of the waves of stimulation in the contractile strips of *Aurelia*—now passing freely, now stopped by an excess of resistance, and now again forcing a passage,—I have felt that I was probably witnessing, on the lowest plain of nervo-genesis, that very same play and counter-play of forces, which, on the highest plain of nervo-genesis, invariably accompanies, if it does not actually cause, the most intricate reasoning of a Newton, the most sublime emotion of a Shakespeare, the most imperious will of a Napoleon, and the most transforming thought of a Darwin.[1]

ATOMS AND EQUIVALENTS

IN the *Comptes Rendus* for the month of May and June there is a series of communications by Messrs. Wurtz and Berthelot containing a discussion of their respective views as to whether chemical changes should be expressed by elements in equivalent proportions or whether the more modern system of atomic weights should be employed.

In the first communication, which is made by M. Wurtz, he remarks on the discrepancy evidently existing between his idea and that of M. Deville, "who has criticised him in a former number of the journal," on the law of volumes of Gay-Lussac. He advances in support of the atomic argument, that free hydrogen may be regarded as a combination of two atoms of hydrogen, the peculiar reaction of hydrochloric acid on hydride of copper and in the case of oxygen with oxygen the reactions discovered by Thenard and Brodie of peroxide of hydrogen on certain oxides. He maintains that the molecular conceptions with regard to bodies in the free state are further upheld, in the case of nitrogen by the formation of nitro and dinitro compounds, and in the case of carbon by the consideration of organic chemistry when examined according to the theory of Kekulé of the grouping of several carbon atoms in the same molecule. After discussing the law of Gay-Lussac as applied to the gaseous compounds of hydrogen with chlorine, oxygen, and nitrogen, he remarks that what results from the previous discussions on this matter, is, that the system of expressing chemical reactions by equivalents which prevailed about 1840 over the atomic notation of Berzelius, has not taken into proper account the discoveries of Gay-Lussac on the combination of gases with each other; and consequently, that the maintenance of this principle in the discussion of chemical phenomena would cause a serious obstacle to the advancement of the science.

M. Berthelot on behalf of those who, like himself, retain the method of writing chemical changes by equivalents, as opposed to the atomic notation, in replying to this first communication of M. Wurtz, states that he does not think the matter to have the same importance which the latter seems to attach to it. He considers that the progress of chemical science is not entirely subordinate to a change of notation which does not strike at the foundation of the science as it had done a hundred years ago to the pneumatic chemistry of Lavoisier. He thinks that at the present day the truths are so general that all the laws may be expressed to a certain extent by both languages with equal clearness and precision. With regard to the view put forward by Wurtz, that bodies in the free state are composed of two atoms, and in support of which view he has mentioned the reactions of hydrochloric acid on hydride of copper, and peroxide of hydrogen on oxide of silver, $Cu_2H + HCl = CuCl_2 + HH$, and $Ag_2O + H_2O_2 = Ag_2 + H_2O + OO$. M. Berthelot deems the explanation given by M. Wurtz mere assumption, without sufficient proof, tending to prevent a true understanding of the real cause of the reaction. He also considers that the true explanation might be found in and explained by certain thermical considerations.

M. Berthelot passes next to a criticism of the atomic method of expressing the reactions of certain metallic salts with each other, and complains of the doubling of the equivalents of certain bodies, such as $CaCl_2$, which he thinks makes an unnecessary complication in the expression of the reactions, and gives as an instance the reaction of certain nitrates with chlorides. By the system of equivalents, he maintains they might be expressed by one reaction—$MNO_6 + M'Cl = MCl + M'NO_6$—but that by the atomic notation four different and distinct reactions are necessary to express their decomposition.

M. Berthelot then alludes to the confusion he thinks has arisen between the words law and hypothesis, in the acceptance of Avogadro's law. In this case he maintains that Avogadro and Ampère have enunciated, not a law, but an hypothesis, in saying, "All gases contain the same number of molecules in the same volume," having, in reality, nothing by which to conceive the idea of a molecule. On the other hand, he thinks the proposition, "The densities of gases or vapours are proportional to their equivalents," being deduced from two orders of properties observable by experiment, may be regarded as a true law. Partisans of the atomic notation have, he considers, substituted for this the proposition, "Molecules of simple gases contain the same number of atoms," and he complains that they thus introduce two hypothetical notions, that of the molecule, and that of the atom. On the other hand, supporters of the system of equivalents say, "Equivalent weights of simple or compound bodies occupy the same volumes;" or the volumes are to each other in the simple ratios, 1, 2, 3, 4, &c., thus :—

1 equivalent of oxygen occupies 1 volume.
1 " Cl, H, or Hg occupies 2 volumes
1 " HCl occupies 4 volumes, &c.

M. Wurtz, on the other hand, replaces the above by the

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[1] Throughout the lecture of which the above is a pretty full abstract, I have associated Mr. Herbert Spencer's theory of nervo-genesis with his name exclusively. To avoid misapprehension, therefore, I append this note to state that I am not ignorant of the fact that the theory in question has occurred to other thinkers as well as to the great English philosopher. Moreover, I am quite aware that even if this theory of nervo-genesis had never been enunciated *à priori* by any speculative thinker, some such theory would certainly have been devised *à posteriori* by any working physiologist of moderate capacity who might first happen to observe such facts as are above detailed. But considering that Mr. Spencer elaborated the theory deductively, and that he did so in a much more thorough and painstaking manner than had ever been done before, considering, too, that he has given the theory so elaborated such a prominent place in his system of objective psychology, I have not hesitated to describe this theory as being pre-eminently a product of his authorship.—G. J. R.

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statement, "All gases contain the same number of molecules;" the molecule may then be formed—

It may be by 1 atom, as Hg, Cd, &c.
„ 2 atoms, as H, Cl, O, &c.
„ 3 „ „ Ozone.
„ 4 „ „ P, A, &c.

What is there in this, M. Berthelot asks, more logical than in his own method? or in what may it be allowed to constitute a modern chemistry in distinction to that of Lavoisier and of Gay-Lussac?

In answer to M. Berthelot, M. Wurtz discusses in a further communication the thermic considerations brought forward by the former in his explanation of the reciprocal reactions already alluded to of hydride of copper with hydrochloric acid and peroxide of hydrogen with other peroxides. M. Wurtz, although acknowledging the value of these considerations, considers the reasoning incomplete, by merely saying that hydride of copper is an endothermic compound; it would be necessary to prove in addition why such bodies, having a greater capacity of heat than their constituent elements, can be formed or exist at all. He maintains that his own ideas admit of the intervention in the thermic considerations relative to the formation of such bodies as peroxide of hydrogen, ozone, chloride of nitrogen, &c., the work done in doubling the molecules of oxygen, O_2, chlorine, Cl_2, and nitrogen, N_2; this work causing an absorption of heat. He brings forward the ideas of M. Favre regarding the formation of bodies with an absorption of heat and alludes to the idea of this latter chemist of representing free oxygen as O_2 and nascent oxygen as O, the latter being more active in consequence of being supplied with more heat from what he calls the "segregation" of the molecule. M. Wurtz concludes that the thermical reasoning instead of being opposed to the atomic conception materially assists it.

In passing to the further objections made by Berthelot to expressing the reaction of potassium and hydrochloric acid as—

$$2HCl + K_2 = 2(KCl) + H_2,$$

M. Wurtz explains it as a necessity to render it comparable to the reaction—

$$2HCl + Zn = ZnCl_2 + H_2,$$

this representing 63·5 of zinc or two equivalents, having doubled the equivalent of zinc, as also of certain other metals for two reasons: firstly, to allow them to accord with the law of Dulong and Petit; and secondly, to satisfy Avrogadro's law. M. Wurtz evidently considers M. Berthelot in the position of almost wishing the abolition of Dulong and Petit's law, and explains that although the product of the specific heats into the atomic weights may not always prove rigorously constant, still they are sufficiently near, it being difficult to obtain one and the same metal under conditions strictly comparable. He wishes therefore to retain the law of Dulong and Petit as a check for the determination of the atomic weights. The notation of equivalents, he thinks, contains certain inconsistencies, and complains that no direct response has been made by its upholders to this point, which is really at the foundation of the discussion; they have only objected to atomic notation as introducing vexatious complications in expressing the reactions of mineral chemistry.

To M. Berthelot's reproach that he has confounded the notion of a hypothesis with that of a law, M. Wurtz replies in the following sentence:—"Je le remercie de cette leçon de philosophie; mais je ne crois pas en avoir besoin." He admits, for his own part, his knowledge that the notion of atoms and molecules is only a hypothesis, one which it is allowable to make on the constitution of matter, and essentially dependent on another, that of the existence of the ether. M. Berthelot believes the atomic hypothesis ill-founded, as atoms and molecules have never been seen, but for the same reason it would be as difficult to imagine the ether. To reject this latter hypothesis it would be necessary to adopt that of continuous matter of differing degrees of density filling all space, and M. Wurtz repeats that chemical notation, the point in question, is independent of such hypotheses.

The notion of atoms and molecules would have to be replaced by that of infinitely small vibrating masses, and at the basis of the notion of equivalent quantities lies the same idea of finite particles. What is really necessary is the choice of exact numbers to express the relative weights of these particles by whatever name they may be called; according to M. Wurtz they are of two orders: isolated, he calls them atoms; combined, molecules. Is this, he asks, an ill-defined idea?

In a discussion like the present, which has existed for so long, and which doubtless will still be continued, it is extremely difficult to balance accurately the arguments used by the upholders of the different theories. Although many and forcible reasons have been brought forward by both contending parties, we fear that no final victory has been gained by either side. M. Berthelot objects to the idea of molecules and atoms, but he evidently does not wish to exclude entirely imagination and hypothesis from scientific reasoning, and deems, as is probably the case, that the fundamental conceptions of the two chemical schools may not differ so much in that matter as perhaps M. Wurtz at present imagines.

The point on which the real difference seems to exist is, in perceiving the true importance of such representative conceptions, and placing them in the position they should occupy in human knowledge. J. M. T.

THE GREENLAND FOEHN[1]

ONE of the chief peculiarities of the meteorology of the Arctic regions, and particularly of West Greenland, is the great variability of the temperature in the cold part of the year. There may not only be a very considerable variation in the average monthly temperature from year to year, but sudden changes from the severest cold to fresh weather, and *vice versâ*, often occur several times in the course of the same month.

Dr. Pfaff has carried on meteorological observations at Jacobshavn for twenty years, and these show that the average temperature of 1872 was −8·7° C., and of 1863 −31·6°, a difference accordingly of almost 23°—an almost inconceivable variableness to us West-Europeans; being, for instance, nearly as great as the difference between the coldest winter month and the warmest summer month at Copenhagen within the same period. The observations referred to also exhibited the most remarkable instances of daily variations. In February, 1860, the thermometer rose on three different occasions more than 25° C. in the course of twenty-four hours.

These singular and sudden rises of temperature almost always stand in connection with a veering of the wind to south-east and east. It may appear very surprising, that the temperature rises with the wind blowing from the high land in the interior of Greenland, which is covered with eternal snow and ice. We need not, therefore, be surprised that old authors have endeavoured to explain this phenomenon by supposed volcanoes in action, or even by a comparatively very mild climate in the interior of Greenland—an hypothesis which it is, however, quite impossible to maintain on meteorological grounds. For every continent in high latitudes must necessarily, from the radiation of heat, be colder in its interior than at the coast where the sea makes the climate milder.

A glance at the map shows that Greenland lies between regions of the earth where, especially in winter, the temperature is exceedingly different. "To the west and

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[1] Abstract of a paper by Hoffmeyer in the Danish Geographical Society's *Journal*, Part I., translated from *Naturen* of June, 1877 (Christiania).

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